Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at http://about.jstor.org/participate-jstor/individuals/early-journal-content.

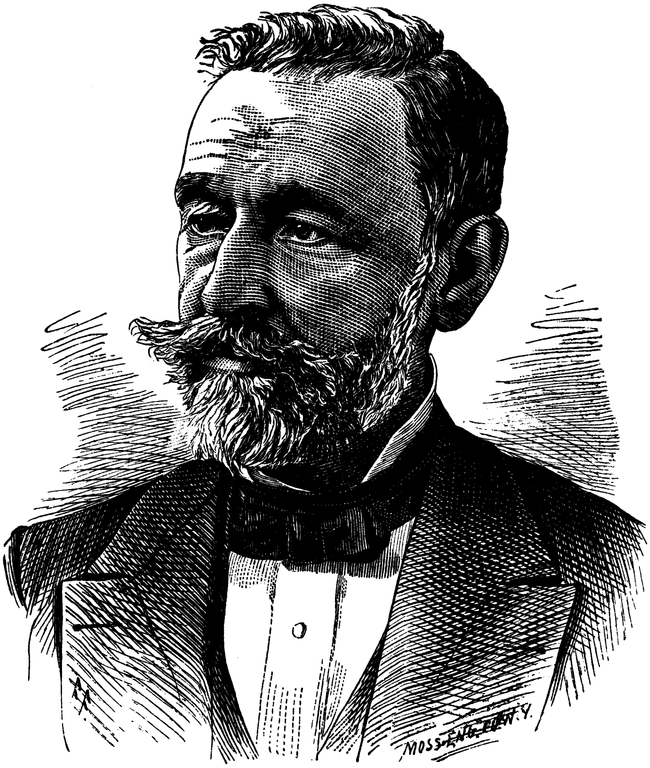
JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

MEMOIR OF CHARLES A. SPENCER.

By HAMILTON L. SMITH, LL.D., F. R. M. S., Geneva, N. Y.

Since our last meeting, one of our most distinguished associates, one who has been justly considered as the pioneer of scientific optics in this country, has been taken from us. In assigning to me the honor of presenting to our Society a brief tribute to his memory and worth, I am aware that the motives influencing your choice are those which have arisen from the knowledge of my long intimacy with our honored associate, and the advantages which a residence in the same village with him during the latter years of his life, and the consequent almost daily intercourse with him, has given me; advantages great, indeed, over any other of our members, yet not out-weighing some that might have been urged in behalf of others. In accepting this honor, I do it with much diffidence, for to render full justice to Mr. Spencer demands a better tribute than I can give. I shall, however, do the best I can in presenting to you a brief synopsis of his labors, continued almost to the end of his life, in advancing the perfection of the microscope and telescope; and, although he has had a cheerfully accorded and world-wide reputation, yet few persons are really aware how much the scientific world is indebted to his persevering labor and ingenuity.

Mr. Spencer was, in the truest acceptation of the word, a genius. Life was not to him a contest for the possession of what the world commonly calls gain; no man was ever more indifferent to this than he. From his boyhood he seems to have had an all-controlling idea,


MOSS ENG. CO. N.Y.

CHAS. A. SPENCER.

Memoir, page 49.

a self-consciousness, which seemed but conceit to those who did not understand him or realize how much there really was in him, of his ability to produce better optical work than the world had yet seen. There is in existence a portrait of him, taken when he was but sixteen years old, and which must have been a very truthful likeness, for it shows clearly the character of the future man. He is looking straight forward with fearless eye, and already reading on the scroll of fame the name of Charles A. Spencer. The bright visions of his boyhood were realized; but he had little conception of what it was to cost him, and that the struggle was only to end with his life.

Almost fifty years have elapsed since Mr. Spencer first began the manufacture of telescopes and microscopes. The only instruments of this character then in the country were imported ones, and probably the whole number of achromatic microscopes was less than a dozen. The names of Powell, Ross, Chevalier and Amici were already known to him, and his aim was to surpass them; but it was the old question : "Can anything good come out of Nazareth?" European microscopists looked upon this country as but a wilderness, and though the labors of Bailey, Torrey, Silliman, and some half dozen others, were beginning to attract tardy recognition in the scientific world of Europe, the announcement that a "backwoodsman" somewhere in the wilds of Western New York had not only dared to question the dictum of England's famous artist, but had actually proved himself right by the constructing of objectives with a largely increased angle of aperture, was received with a smile of derision and considered as a Yankee fiction. The merits of Mr. Spencer were however soon candidly acknowledged, and with him commenced that steady improvement of the microscopical objective which has continued to the present day. To no one person has it been given to work out all possibilities. Ross had already introduced the beautiful cover correction; Powell had shown himself a master in the figuring and mounting of minute achromatic combinations; Amici had suggested the immersion principle, though with no conception of the immense advantages to be gained by it—rather as a substitute for the cover adjustment of Ross; but no one had yet seen the way to any considerable increase of the angle of aperture, or indeed, had realized the benefits to be derived from it. Mr. Spencer, happily, lived long enough to see the triumph of principles advocated by him, and first by him successfully carried into execution.

I have said that Mr. Spencer was, in the best sense of the word, a genius; if he had been anything else he would have accumulated a fortune. An intimate friend of his assures us that when in the midst of his fame at Canastota, the unfilled orders on his books were often $20,000 to $25,000 in advance—orders from all parts of the world, but alas, many of them, such is genius, never filled, for Mr. Spencer was never quite satisfied with his work. No matter how perfect it was, for that time; no matter that it would have more than satisfied those for whom it had been done, and who had already waited patiently for it; if it did not come up to his ideal, or if he could possibly see that any improvements could be made, conditions constantly occurring, what to him was time, what was money, nay, what were all his promises, what was anything, compared with this yet to be accomplished higher excellence? And so it happened that very often it cost him much more to produce a given piece of work than the pay he received for it. Large as were the prices which his acknowledged skill enabled him to obtain,—prices which were only too willingly paid, so the work could be had at all,—yet Mr. Spencer's life, from boyhood, when with enthusiasm of youth he saw a name and fame opening before him, up to the time when, enfeebled by age and disease and apparent neglect, he entered into the eternal rest, was one long struggle with poverty. Not from want of industry. No man was ever more industrious, but not in the way of the world. There is little money to be made in the tedious testing and skillfully touching up what any one else at the time would have called perfect work, but which the artist is unwilling to let out of his hands, unless stern necessity compels, so long as he can imagine something better; albeit to accomplish this better, may be the work of days, weeks, or even months of trial and severe application. All this in the eyes of the world, is of no pecuniary value, and business men have little sympathy with genius.

Few of those who have in past years accomplished much in the way of perfecting instrumental means of scientific research or the advancement of the economic arts, have, during their life-time, received any adequate reward, and certainly Mr. Spencer was not an exception. He might have gathered a more abundant harvest of what the world terms wealth if he had understood and practiced somewhat closer the ways of the world in financial matters; but where, then, would have been the brilliant results? Mr. Spencer wasn ot only industrious, though not in a way for pecuniary recompense; he was also economical, and of unblemished moral character,—for we must not consider that the unfulfilled promise, the often apparent and blameable neglect, arose from any intention or design on his part to say or do aught wrong. He had an unbounded hope,—it was a moral certainty to him, a faith in himself of being able to do all he had promised, and of accomplishing all he might undertake,—which only lacked prudence in the ways of common life, and a closer watching of the balance sheet, to have been fully justified. He has gone,—he has left only friends. He had outlived most of those who hailed his advent as an honor to American science; and in the rapid march of improvement in the past decade, he had begun to feel, with his infirmities and declining powers, that he was almost left behind. We cannot say he had not grave faults,—few of us but have; happy if, when we lie down to rest, so few will be remembered of us. When the name of many a successful man, as the world counts success, shall have been forgotten, and the marble on which alone it is recorded shall have crumbled away, that of Spencer will still live; nor will it be forgotten until the human eye no longer needs a microscope, but shall see clearly the now hidden things of God.

In preparing a sketch of the life and work of our late associate, I regret that the materials at my command are so meager. Again and again I had urged him, during the last years of his life, to put on record an authentic account of his labors and the particulars of his life, and I believe he did commence this desirable work; but if so, no trace of it has yet been found. Mr. Spencer had a most wonderful memory, and he had found that he could trust it so completely that he felt independent of written notes. He could recall events, and narrate particulars of past years, with all the freshness of the present; nay, he could give the very words of his conversations with scientific men of this country and Europe, now long dead, with wonderful ease, and his fund of anecdote was inexhaustible. It would have been of immense advantage if I could have consulted the voluminous correspondence of Mr. Spencer, written before these days of "commercial note," when letters really meant something; but all these, and numerous interesting papers bearing more or less upon his early life and labors for the improvement of the microscope and telescope, were destroyed in the disastrous fire at Canastota, whereby Mr. Spencer lost not only his whole stock of tools, lathes, and delicate machinery, but much finished and unfinished work. The correspondence to which I have had access is very limited, and I can present but little in addition to what has been already published.

Mr. Spencer was born on Quality Hill, in the town of Lennox, N. Y., in the year 1813. He was the youngest son of Gen. Ichabod S. Spencer, and nephew of the late Joshua A. Spencer, of Utica, N. Y. He came from Lennox to Canastota about 1831, and to Geneva in 1875; and died at the latter place September 28th, 1881, at the age of sixty-eight. Mr. Spencer was educated at the Cazenovia Academy, and after graduating there he entered the Freshman class at Hobart, then Geneva, College, at Geneva, N. Y.; his uncle, Dr. Thomas Spencer, being at that time a professor in the medical department of the college. He remained at Geneva, however, less than a year, and soon after went to Hamilton College, Clinton, N. Y., of which his uncle, Judge Joshua A. Spencer, was then one of the trustees. His repugnance to being educated at the expense of others, or as a "charity student," as he termed it, was so great, that he did not long remain at Hamilton, but returned to Canastota to study and experiment by himself; as he had found that at this early day comparatively little attention was paid in the colleges to the subjects which most interested him, most of the time being devoted to classical studies. He betook himself, therefore, to more earnest practical work in science; not, however, neglecting to give a fair attention to classical literature; and in later years he received the honorary degree of A. M., from Hamilton College.

The first appearance of Mr. Spencer before the public as a manufacturer of telescopes and microscopes was announced by a circular printed upon a sheet of the large-sized letter-paper of the day. No copy of this circular now exists, so far as I am aware, but the date must have been near 1838, as I well remember perusing it with wonder and admiration during my last year at college. It was a list of prices of various sized reflecting telescopes, chiefly of the Newtonian and Gregorian construction, and of Dr. Goring's "engiscope," or reflecting microscope. If any achromatic telescopes or microscopes were named, they were but small and considered inferior to the reflecting instruments. Of the reflecting telescopes, Mr. Spencer appears to have made quite a number, and a few of the "engiscope." I have never seen any of them, though no doubt some of them are yet in existence. About two years after this, Mr. Spencer issued a descriptive catalogue, "Optical, Philosophical, Mathematical, Chemical and other Instruments and Apparatus." At this time he was in partnership with his cousin, Hamilton Spencer, son of Dr. Thos. Spencer, though only Mr. Chas. A. Spencer's name appears on the catalogue.

The telescopes are chiefly reflectors, from 10 to 3 inches aperture, and ranging in price from $8 to $75. The achromatics are from 1 to 3 inches in diameter, and prices from $12 to $200. The "Amician Reflecting Microscope, with complete apparatus for transparent and opaque objects, a micrometer for measuring to the ten thousandth of an inch; six powers, from 80 to 400 in lines, and 6,400 to 160,000 in surface," with "all necessary coarse and delicate adjustments, packed in a mahogany chest, to which it may be affixed, when in use, as a stand, with twelve brass or ivory slides for common objects, and three for liquids," is set down at $200; and the "Compound Achromatic Microscope, with complete apparatus, four object- and two eye-glasses to vary the power, the glasses all achromatic, powers from 20 to 400 in lines, and 400 to 160,000 in surface, with micrometer, apparatus for enlightening, and twelve object-slides in brass or ivory; the whole in a mahogany case, which serves as a stand when used,—$200. In a foot-note, relative to the powers of these microscopes and the use of tests as proposed by Dr. Goring, the tests named are scales of the white cabbage butterfly, the podura, and the small brown moth. Besides these the catalogue enumerates various solar, lucernal, botanical, pocket, and other microscopes, and various mechanical, hydrodynamical, pneumatical, electrical, meteorological, galvanic, pyronomical and chemical instruments.

I believe the business did not prove to be as profitable to the young aspirants as they had hoped, and the partnership was soon dissolved, Mr. Chas. A. Spencer, however, still devoting himself to the perfection of the achromatic microscope and telescope, which seem now to have chiefly engrossed his attention; and probably his experiments were more in the way of making achromatic telescopes than microscopes. There is no doubt that the study of astronomy was particularly fascinating to him, and his fondness for the telescope and telescopic pursuits never diminished, though he found in the development of the microscopical objective a more promising field for his genius. It cannot be ascertained just when or by whom his attention was first called to the possible capabilities of the achromatic combinations for microscopical objectives. The old-fashioned triplets as first made by Chevalier, and better by Oberhauser, were those chiefly used. Powell and Ross had indeed improved on these, but there were few in this country who had their glasses; and, indeed, in 1847, when Spencer made for Dr. Gilman, of New York, the first microscope that attracted attention, there were not over four or five achromatic microscopes in the city of New York. Dr. Gilman, having showed to Mr. Spencer one of these, a Chevalier, Mr. Spencer remarked that he thought he could make a better one, and the doctor decided to let him try. It was accordingly made after the same pattern, and Mr. Spencer took it to New York, stopping on the way to see Prof. J. W. Bailey, at West Point, and this was his first interview with this celebrated microscopist. At this time he seems not to have known the use of the *diatoms* as test objects, though he had heard something of them, and called upon Bailey for the purpose of having him try the new microscope upon these objects. The powers which Mr. Spencer had made, probably imitating those of the Chevalier, were a $\frac{1}{3}$, a $\frac{1}{7}$, and a $\frac{1}{12}$ in. There were a lieberkuhn for the $\frac{1}{3}$ in., and polarizing apparatus. The following extract from a letter written by Mr. Spencer to Dr. John Frey, of New York, dated Canastota, N. Y., Oct. 21, 1847, and published in the *American Monthly Microscopical Journal*, October, 1881, describes the interview in the following language:

"Now, between ourselves, I did *not* make as good an instrument (in the highest power, I mean) for Dr. Gilman as I *know* I can make. I used the Swiss flint, with the intention of beating the foreign instruments with their own materials—with the design of replacing the highest power by one of my best at a subsequent day. * * * * I wished to get, moreover, as correct an idea of the capacity of the foreign instruments as possible. Aiming to make Dr. Gilman's instrument as nearly the standard of the best as I could judge, and knowing that Prof. Bailey's *Navicula hippocampus* test would prove the matter well, I called upon him. * * * * I had some misgivings, of course, as to the resolution of the *hippocampus*, and told Prof. Bailey so. * * * * I then set up my instrument and showed him, as the first object, the scales of the *Podura*. He almost instantly exclaimed, with warm earnestness: 'I am sure that it will show the *hippocampus*.' I replied that I feared not, as I had not one of my best highest powers with the instrument. Prof. Bailey insisted upon finding the object on the slide, and the instant his eye caught it, he exclaimed: 'It shows it beautifully—this is a perfect treat!' I then looked at it, and must confess almost instantly lost my reverence for the English test object *par excellence*."

The test object above alluded to,—*Navicula hippocampus*, now called *Pleurosigma hippocampus*, and really the marine form of *P. attenuatum*, and easily resolved by a good ½ in. or even ⅔ in. objective,—seems to have been first proposed as a test in the year 1841, and the specimens of Prof. Bailey, sent by Prof. Williamson, were from Hull, England; and the following statement in the *Microscopical Journal and Structural Record*, 1842, page 160, and signed Robert Harrison, Hull, 6th of June, 1841, will show how difficult it was then considered: "It is not generally known, and, indeed, it has been denied by some, that there exist longitudinal striæ in *Navicula hippocampus*. In specimens of recent *Infusoria*, collected by me at Hull, several of this species are met with, and the striæ can be distinctly seen, with a power of about 400 linear, especially if not mounted in Canada balsam." The editor observes that, while many of the specimens are beautifully striated longitudinally, there are some which show no signs of such marking.

In the following number of the *Journal*, page 224, the editor writes: "In our last number we noticed the existence of distinct *longitudinal* striæ on this species of *Navicula*. Our attention has since been directed to *transverse* striæ on the same species, by Mr. Harrison, of Hull. We can, with him, testify their presence, after some considerable patience and manipulation as regards the modification of the light. They are more evident in the single shell, not mounted in Canada balsam, than in the slide in which they are set up in that menstruum." Prof. Bailey gives a glowing account of the performance of the microscope made for Dr. Gilman, in *Silliman's Journal*, vol. V., second series, 1848, in a letter to the editors under date November 27th, 1847. He states that Spencer's lenses are far superior to any of Chevalier's which he had yet seen. He says: "With Spencer's glasses I was able, without difficulty, to see the cross-lines on the *Navicula hippocampus* (the most difficult test object now known to me, and which was sent to me from England as *the test object par excellence*); while with Chevalier's, by the same light, I could see them only with great difficulty. I do not think Spencer's microscope quite equal to the far more expensive English ones made by Ross or Powell, and I cannot positively assert that it is superior to the best of those made by Oberhauser or Plœssels, as I have not had an opportunity to compare them properly side by side in the same light. My impressions, however, are that the microscope made by Mr. Spencer for Dr. Gilman is in no respect inferior to any of Oberhauser's that I have seen, and I am by no means certain that it is not superior. I certainly had a better view by *candle-light*, of the transverse lines of the *N. hippocampus*, by means of Spencer's instrument, than I was able, subsequently, to do by *day-light* by means of Oberhauser's. * * * I look upon the results obtained by Mr. Spencer as a proud triumph for American art."

The same article in *Silliman's Journal* includes the report of a committee to the New York Lyceum of Natural History, signed by John Torrey, J. F. Holton, and John L. Le Conte, from which I make the following extract: "Your committee have been able to subject the microscope made by Mr. Charles A. Spencer to a rigorous comparison with an instrument made by Chevalier for one of your committee (Dr. Torrey), and supposed to be one of the best instruments of French make ever imported into this country. The superiority of Mr. Spencer's microscope over Chevalier's Number 1 *is very decided*, although made on a much smaller and less expensive scale." From an appendix to the same article, but dated later, West Point, February 4th, 1848, we learn that Mr. Spencer, at the interview alluded to with Prof. Bailey, made him this promise, after seeing the *Navicula hippocampus* for the first time: "I will make you," said he, "an objective which will show the lines in spite of the most awkward illumination." This pledge, Prof. Bailey states, Mr. Spencer had fully redeemed, and he says that the objectives, then in his possession, would not only easily surpass any of French make, and do all that the very best English objectives could do, but more than this, he states that by means of them he has resolved the lines on a *Sigmoid navicula* which he had found in the Croton water of New York; an object compared with which, as Mr. Spencer, who seems first to have resolved it, had said, "all other test objects are mere child's play," and which Prof. Bailey calls *Navicula Spencerii*.

This diatom is now known as *Pleurosigma Spencerii*, and as a test is fully described by Prof. Bailey in *Silliman's Journal*, vol. VII, 1849. It is readily resolvable now by a first-class ½ inch, and by any ordinarily good ½ inch. Prof. Bailey sent this diatom to Mr. Spencer in December, 1847, telling him that he *must* resolve at least two sets of lines upon it, though he himself could do nothing with it with his Chevalier lenses. Mr. Spencer thus replies: "I find the *Navicula* is striated, as you supposed, longitudinally and transversely. It required all the means and appliances I could command to resolve it. *But I hope the powers I am making will tell a different story;*" and shortly after he sent to Prof. Bailey an objective by means of which the latter also easily succeeded in bringing out both sets of lines upon this test.

At this time Drs. C. A. Beck and Paul Goddard, of Philadelphia, were possessors of the best work of Powell, and each had a $\frac{1}{12}$ in. objective, considered by them the very best in the world. Dr. Gilman had already compared the objectives of the microscope made by Spencer for him, with Dr. Goddard's Powell lenses, and announces the result in a letter to Mr. Spencer, dated New York, May 6th, 1848, as follows:

* * * * "I saw and tried Goddard's $\frac{1}{12}$ made by Powell. It is a glorious glass. Magnifies, I think 850 (diameters) and shows the *Nav. Baltica* superbly; not, as I think, with any more distinctness, and I think not with as much neatness as my highest powers, but very much more easy to find, and very striking. This day Beck

called at my house and examined my microscope. He admired it much, and admitted frankly that it was better (far better) than any Chevalier or any Continental microscope he had ever seen. He was delighted with the $\frac{1}{8}$, and made it bring out the lines on the *Eng. Podura* well. The next, $\frac{1}{7}$ I call it, he liked even better, and admitted that his lenses were to run a bad chance of failure. But he says his $\frac{1}{12}$ is better than yours, and to tell the truth *I think so too.* * * * * Beck goes to West Point, Monday or Tuesday. (This was early in May, 1848.) His glasses will do nothing with the lines on the *Nav. Spencerii.*"

By some unaccountable blunder, Prof. Bailey appears to have estimated the transverse lines of this diatom at from 120,000 to 200,000 to the inch—finer than any diatom test now, *Amphipleura pellucida* averaging not far from 100,000 to the inch. Mr. De La Rue had already determined these markings more correctly as 40,000 to the inch; but, meantime, while Mr. Spencer was aiming at still higher perfection, Prof. Bailey sent the *Nav. Spencerii* to microscopical friends at London, with a statement of what Mr. Spencer's had done, but does not seem to have instructed them as to the necessity of a more oblique illumination than many of the stands then in use would admit, and it was like exploding a bomb-shell in the enemy's camp; the name of Spencer became at once famous.

The result of Dr. Beck's visit to West Point is thus announced in a letter from Prof. Bailey to Mr. Spencer, dated May 20th, 1848: "I have news for you which will make you jump about your shop like a kangaroo when 'stirred up' by the pole of a menagerie keeper. Dr. Beck, of Philadelphia, has been here, and for the express purpose of seeing the lines on the *Navicula Spencerii*, 'if they were to be seen.' I have had him in the den, and we have looked together, night and day, for two or three days. He is delighted with your work, and it did not take long to convince him that both transverse and longitudinal lines on the Croton test had a real existence. He became convinced that *direct* central light would not do for this class of objects, and that the only way is to follow the advice of old Polonius, and 'by *indirections*, find *directions* out.' He admits that there was *some* scepticism in Philadelphia about the existence of these lines. The point, however, which is to stir you up is this. His $\frac{1}{12}$, made by Powell, was attached to my Chevalier, and tried first on *Podura*, and compared with yours. Beck was not quite satisfied with the result for your lens. * * * But you want to know whether his lens resolved the Croton. It did; and *handsomely*, too. We tried it on several specimens, and by strong light, medium light, and by candle-light; in all cases the performance was beautiful. The lines on all the test 'scamps,'[*] Balticas, Stauronei's and Croton pets all came out easily, and with a light less oblique than your lenses required. The power of his is greater than yours, although he calls it a $\frac{1}{12}$. It magnifies considerably more than your $\frac{1}{16}$. How do you account for that? * * * It does not lower your position at all to admit the excellence of Powell's lenses; you have only a 'bait ahead' to see if you cannot really beat him. * * * Our friend Cole will be vexed to see that a foreign microscope has got the start of his in resolving the Croton. I fully believe that both his and Mr. Lowell's instrument will accomplish this feat. Beck's will master the '*dumpy Sigmoid;*' so we must look for something still more transcendental to drive the Powells and Rosses off the track."

The English microscopists were now experiencing the same difficulties which had prevented Drs. Beck and Goddard and Mr. Thomas Cole, of Salem, Mass., from resolving the "Croton" with objectives which, upon the whole, as we must now concede, were quite equal to any Spencer had yet produced, perhaps better. It is delightful to read at this late day the few letters which remain of the voluminous correspondence which must have been carried on between the early microscopists, when the new powers of the microscope were just being unfolded, and a whole world of original investigation, full of marvels and wonders, was opening before them. An almost boyish enthusiasm appears to animate them, and is apparent in their familiar correspondence; and I make no apology for quoting *verbatim* the following letter of Prof. Bailey to Mr. Spencer, dated West Point, June 20th, 1848. We can imagine it was written on a bright summer day. Prof. Bailey, with his large Chevalier near by, and slides and papers scattered around, was sitting at his table in a morning-gown which would be considered quite æsthetic now, with its large flowers and palm-leaves. Alas, few are left who remember it. The mail, then not an every-day affair from England, has arrived, and now a second time the "Croton pets" are heard from. This letter, written instantly to Spencer, announces the result:

---

[*] This was the name merrily given to the *Pl. hippocampus* by the microscopists who found it gave them so much trouble.

WEST POINT, June 20th, 1848.

My Dear Sir:—Our striped pig does squeal most delightfully. He beats the "Learned Pig" all to nothing. His tail is too well greased for even John Bull to catch hold of it. Just hear what my London correspondent, Mr. Marshall, says of it: "On the same evening when your package was put into my hands there happened to be a small gathering of our microscopical friends, and your slide underwent a long examination. We, however, could make nothing of it. I would be glad to have you send me a number of specimens, to set my friends at work on this interesting point, as your statement about Spencer's glasses is exciting some sensation here. Please also let me know Spencer's charge for his glasses, as I should like to get one over, if the cost is not too heavy. We had some of the finest glasses of Smith, Ross and Powell in our examination of the *N. Spencerii*, and I am bound to state that at present the result is most unsatisfactory. Since the foregoing was written I have made several efforts to get at the markings on the *N. Spencerii*, but without success, I shall therefore feel most particularly obliged by your directing your "backwoodsman" to make me one of his best object glasses. I send in a small separate-parcel (not yet received) the gauge of the screw which it is to fit. I presume your ingenious artist will not object to his reputation being extended beyond the broad Atlantic. I hope he will put it in hand at once, so that I may have it at the earliest period compatible." In a subsequent letter of June 2d, he says: "Still, however, I can make nothing of your *Navicula Spencerii*, although I have employed one of Powell's best glasses, with the power of at least 1,110. On Wednesday last was the monthly meeting of the Microscopical Society, and on that occasion I read to them your letters respecting Spencer's object-glasses, with which they were much interested. Ross was present, and they all expressed themselves much gratified to learn that we are likely to obtain a glass from America for comparison. You may rest assured that full justice shall be done, and in the event of its proving as yours is, there will be no doubt about further orders going out. You of course are aware that our best objectives have movements attached to them to adjust them for covered or uncovered objects. This is a very important point, which doubtless Spencer has considered. With respect to Chevalier's objectives, the London microscopists would hardly take them as a gift. Perhaps you will kindly inform me about the period at which I may look for Spencer's objective."

Is not all the above particularly delightful? Verily, the good Cockneys need the ghost of Goring to teach them how to put the *proper objects* at each end of their microscopes. I wonder if they believe in my "statements" at all? What shall I tell Marshall about the objective which he has ordered? Don't send anything over until you are *sure* of astonishing the natives. When you have anything ready for Marshall, such as you are willing to submit to the criticism of such rivals as Ross, Powell and Smith, send it to me and I will forward it safely to Mr. Marshall. In the meantime let me know as soon as you can what answer to make to his request. I shall write to him soon and tell him of the success of the Philadelphians with their Powell and Ross microscopes. They have got to swallow a bitter pill anyhow they can fix it. They have either to admit that their instruments are inferior, (which as yet I don't believe), or else that they do not know how to use them. I wonder they have not seen the "longitudinals" which our friends Cole and Gilman find so easily. By the way, our friend Cole has almost blinded himself, since I informed him of Beck's success. He insists upon it that he *has* seen the longitudinals! but he cannot yet see the transversals. I trust that he will follow Beck's example, and come on here to see these "pests of microscopists," as the heretics call our pets.

Write soon, and don't fail to tell me what to say to Marshall.

Yours with hearty good-will,
J. W. BAILEY.

What can I say to Bartlett and Remble about their lenses?

Of course, it is hardly necessary to say that, when the proper illumination was applied, the English microscopists easily resolved the "Croton test;" but already Mr. Spencer was prepared with one far more difficult.

In another letter, written a few days before to Mr. Spencer, Prof. Bailey advises him to send on an objective and eye-piece to Mr. Cole, at Boston, as possibly worthy of the Rumford medal; and in reply to some objections which Mr. Spencer seems to have urged as to his unwillingness to enter into competition for a prize, he says: "I hope you will get the Rumford medal. As for appearing as a *candidate* for it, I would do no such thing. It appears to me that all you have to do is to complete, as soon as possible, a good objective and eye-piece for Cole, and let him show them to the Bostonians. If they choose to give you the medal, well and good. I would make no other effort to get it. If Cole should fail to show the lines on the Croton test with your lenses, I will go myself, and let the Bostonians see that some things *can* be done, as well as others."

Among the New York microscopists who were attempting to make something out of the *Nav. Spencerii* with French instruments, was a Dr. Stout, and in this letter to Mr. Spencer just mentioned, is the following amusing doggerel, which, though never intended for the public eye, I trust I am not doing any wrong in quoting:

"It seems that Stout has not seen the striped pig yet.

Oh, what an offence, sir,
That you, C. A. Spencer,
With your little lens, sir,
Should bring the lines out, sir,
Which the great Dr. Stout, sir,
Can do nothing about, sir,
Though he makes such a row, sir,
With his Oberhauser;
'T is a pity, I vow, sir!"

Before the reply had been received of the success of the English microscopists in resolving the *Nav. Spencerii*, a feat which, like that of the egg of Columbus, was easy enough to be performed when once one knew how, Mr. Spencer announced the discovery of a still more difficult test, and of such improvement in his objectives as enabled him to resolve it. He was now fairly a rival of the best foreign artists, and acknowledged by them as such. Practically and experimentally, he had gone over the whole ground, and was now obtaining wider angles than any one else, and yet with excellent corrections of the chromatic and spherical aberrations; and we are told that Mr. Ross, in speaking of the subject, said he thought that Mr. Spencer must have some mode of working glass as yet unknown to other opticians. And this was the case, for, although he did, about this time, make extended and costly experiments in the attempt to produce a glass of higher dispersive and refractive power than was then made for sale, and was, to a certain extent, successful, so that many of his best objectives at this time and even at a much later date were made with his own glass, numerous specimens of which are still in existence, yet his chief success was from his tact in *figuring the lenses* so as to balance the aberrations, a process so delicate that it would have availed no one not possessed of the same skill to copy the curves, even if this could be done, and with the same material. The new test was a variety of *Grammatophora marina, Ehr.*, and it was called, provisionally, *G. subtillissima* by Prof. Bailey. In November, 1848, Mr. Spencer produced a $\frac{1}{16}$ which resolved this diatom as found in gatherings from Greenport, L. I., and it proved too difficult for any foreign objectives then in this country. It is a test still for a first-class dry $\frac{1}{4}$ or $\frac{1}{8}$; and a yet more finely marked form of the same diatom, from Providence, R. I., is considered now a good test for any dry objective.

From this date Mr. Spencer seems to have kept steadily in advance of foreign opticians as to angle of aperture, and in June, 1850, he produced a $\frac{1}{12}$ having the then marvelous aperture of $174\frac{1}{2}$°, as measured by the old Sector method. This objective was exhibited to a number of gentlemen who met at Canastota, and who each desired to possess it. Judge Alexander S. Johnson, of Utica, N. Y., became the fortunate possessor, out-bidding the rest, and he states that it would dot the Greenport *Grammatophora.*

While Mr. Spencer was thus steadily increasing the angle of aperture of all his objectives, that of the $\frac{1}{4}$ being 100° or more, the English microscopists appear to have deprecated this, and to have had serious doubts whether much more increase of angle than their objectives already possessed would be of any advantage—nay, whether it might not be of positive detriment; and Mr. Ross, we are informed, had concluded that 135° was the possible limit of usefulness. (See Quekett on the Microscope, London, 1848, p. 431.) In 1851 the Jury, Class X, of the Great Exhibition, reported "Combinations of high power and extremely extended angles of aperture, are excellent in developing one class of objects, viz., minute lines or dots on plane surfaces, and admirably demonstrate the high perfection to which such glasses are capable of being carried by scientific opticians; but such combinations, with a less angle of aperture and more penetrating power, are far more generally useful and valuable to the minute anatomist and the naturalist;" and in 1853, the President of the Microscopical Society of London, George Jackson, Esq., says substantially the same, and at this time the maximum angle of Ross' $\frac{1}{12}$ was 135°, and of the $\frac{1}{8}$, 107°. To show that the large-angle objectives of Spencer were not considered by naturalists and biologists as merely useful for bringing out certain lines on plane surfaces, I need only refer to the work of Dr. Riddell, at New Orleans, and his researches on blood, published in 1852, and which were made with a $\frac{1}{12}$ of 170° angle, and of which he says: "It is the best he ever made, and though charging me only the moderate sum of $120 for it, yet its defining power is so great and so wonderfully accurate that a sum of money greater than I choose to name would not deprive me of its possession."

Fortunately, the reply of Mr. Spencer to the report of the jury above alluded to is in existence. It is so interesting both as to its subject-matter and as illustrating the clearness and soundness of Mr. Spencer's views at this early date, that I do not hesitate to give it entire. It is apparently an article by Dr. Dwinelle, of New York, on "Microscopes of Large Angle of Aperture," but was written, with the exception of only one sentence which I omit, by Mr. Spencer himself. It was published in January, 1853, and reads as follows: "Since the discovery of Dr. Goring that the penetrating powers of the microscope depends directly upon the angle of aperture of its objectives, all judicious and successful artists have made this discovery the basis of their labors for improvement. All the later advances which have been made have given additional evidence of the truth of this principle. Indeed, the discovery of Dr. Goring may be considered as an established law, founded on evidence as complete as any law of physical optics. In the report of the jury of the great exhibition on microscopes, and in the published opinions of some microscopists of the present day, especially English ones, we have observed an indirect doubt expressed as to the truth of this law. That objectives of great angle are of little value for viewing other than the ultimate surface of objects, that they are entirely wanting in penetrating power, and that for the examination of objects of structural anatomy, or wherever a depth is required, an angle of aperture of 75° is superior to all others, would seem to be the general opinion abroad. Thus the report and opinions referred to agree in the statement that 'it is advisable that the angle of aperture of the combination should not be extended to its utmost possible limit, when destined for general purposes of natural history or anatomical investigation. Combinations of high power and extremely extended angles of aperture are excellent in developing one class of test objects, viz., minute lines or dots on plane surfaces, and admirably demonstrate the high perfection to which such glasses are capable of being carried by scientific opticians; but such combinations with a less angle of aperture, and more penetrating power, are far more generally useful to the minute anatomist and naturalist;' that is to say, we must *diminish* the angle of aperture and *increase* the penetrating power, when it is a perfectly established law that penetration is absolutely dependent upon angle of aperture! So true is this that *penetration* and *angle of aperture* are convertible terms."

It may be observed in passing, that the term *penetration* is an unfortunate one, as every such term must be that admits of doubtful or double meaning. Some have undoubtedly understood by the term, a certain latitude or *depth*, within or without the best focus, by which slight elevations or depressions, below or above the general surface of an object, remain visible and comparatively distinct. It is hardly necessary to say that this understanding of the term is an entire mistake.

By penetration is intended that power by which faint or minute markings are revealed, markings which remain invisible to the most perfect *defining* and *magnifying* powers when unaccompanied by penetration. A more appropriate term would be *resolving power*.

The existence of latitude or depth of focus, above referred to, is perfectly incompatible with the highest excellence in an objective, and the presence of such a characteristic is ample proof of imperfection in the correction for sphericity. Such objectives cannot give perfect definition, and are invariably blind to the fine markings of objects. The focus of a perfect objective may be fitly called a mathematical plane—extension without depth or thickness; and the more nearly its practical operation coincides with this definition, the more nearly perfect it may be assumed to be.

But to return from this digression. No reasons have been given why the conclusions above named (against the extension of the angle of aperture) should be received. So complete a departure from the path in which all past improvements have been made, would seem to have required this. Opinions, coming from such a source, always carry a certain weight with them, especially with those who are ignorant of the principles involved, and who must, therefore, take their judgments at second-hand. In this instance, the declared conclusions are utterly at variance with both theory and experience. In every instance in which an extension of the angle of aperture has been effected, without the introduction of defects in the corrections for color and figure, the increased efficiency of the microscope has become at once evident and undeniable. We must seek for an explanation of the erroneous conclusions of the jury in something besides the destruction of an established law and a denial of the results of all experience.

Fortunately, the explanation is not so difficult as might be supposed. It may be presumed that the attempt to increase the angle of aperture of any given objective implies the necessity of abandoning all, or nearly all, of the curves previously used in its construction, and in many cases an entire change of material. The addition of a very few degrees to any given angle of aperture so changes the thickness and arrangements of the component lenses of an objective that the balance of corrections for color and figure is entirely destroyed. It follows from this that each objective, of given angle, requires an independent investigation of its curves and combinations to produce the best effect; and it may be added that after the most searching analytical investigation there will always remain some practical defects, which can only be corrected by practical means. We can readily understand, therefore, that there is a period in the progress of improvements, during which imperfect results are likely to be obtained. The practical defects mentioned can only be remedied after repeated trials, and as the artist cannot afford to bear either the loss of time or the heavy expense involved in such experiments, it follows that his work must leave his hands in an imperfect state during such periods. The investigations undertaken by Spencer in this country, and which resulted in the extension of the angle of aperture from 135° to 175°, will serve to illustrate this subject. Although an angle of 170° was obtained by him at an early day, thus proving that the limit of 135° assigned by Mr. Ross was erroneous, yet so great a sacrifice of the working focus and of the definition was the result of the earlier formulas, that the artist refused to permit any such objectives to leave his hands. Nearly two years elapsed before these defects were so far overcome as to make such powers practically available.

The results of a recent examination of some of the latest and ostensibly the best objectives of Ross, and Powell and Lealand, will probably enlighten us still further as to the probable grounds of the conclusions arrived at by the jury. The objectives were two of one-fourth of an inch by Ross; and a half-inch, a one-fourth and one-twelfth by Powell and Lealand. The two objectives of Ross differed in their angles of aperture, one of them having the angle usually obtained in this power by the maker, was characterized by the usual skill of the artists in the fine balance of its corrections. The other, of greater angle of aperture, was defective in the corrections for both color and figure, and the difference between the two objectives was so considerable that the owner* (though not understanding the cause) had early arrived at a correct conclusion respecting their comparative excellence, declaring the one of lesser angle decidedly the more effective of the two. A careful examination of the deficient objective revealed the fact that the artist had endeavored to increase the angle of aperture mainly by increasing the diameter of the compound lenses. Like all such attempts it was a failure, a result that safely may be predicted in all cases of the kind, however gifted the artist who makes them. The correction for color was not glaringly defective, but the corrections for spherical abberation were very far from being balanced, a marginal over-correction and a central under-correction were at once apparent, and of such amount as seriously to impair the definition of the combination. If such an objective had been brought before the jury of the exhibition as one of unusually great angle of aperture, and especially as one of Ross' best (as the owner declared was stated to him), we can readily imagine what would be the result of the examination. The jury, relying on the acknowledged skill of Mr. Ross, and believing with the peculiar faith of Englishmen that the work of their most distinguished artist must be perfect of its kind, would most surely have arrived at a judgment adverse to an increase of the angle of the aperture in the power under examination, without further warrant for such a judgment than was supplied by such a faith in the artist and the evident deficiencies of the objective.

The half- and quarter-inch objectives of Powell and Lealand, were highly creditable to their makers, the quarter-inch being unusually good. They were of the usual angle of aperture of such powers. A careful examination of the one-twelfth showed that the angle of aperture had been increased at the expense of the definition. It was easily to be seen, also, that the formula used in its construction was mainly a simple reduction of the curves of the quarter-inch. The artists had apparently been prompted by the excellence of their one-fourth objectives to try the application of the formula to the one-twelfth. The result, as above stated, was that the objective, though of larger angle of aperture than usually obtained by them, was defective in its corrections for spherical aberration, or in other words, in its definition.

---

* **Prof. Bigelow, of Boston.**

If we should assume that these objectives are fair examples of the skill of their respective makers, we might with apparent good reason, arrive at conclusions identical with those of the jury; but in so doing we should be alike unjust to the artists and forgetful of the true principles upon which to form a correct judgment. It does not by any means follow, on the one hand, that the artist must be always unsuccessful in his difficult task, and, on the other, we are not at liberty to cast a shade of doubt on well established laws, because of our inability to separate the deficiencies of the artist from the sound deductions of theory.

Certain it is, that no solid reason can be given against the extension of the angle of aperture of objectives to the greatest possible limit—and for every power—consistent with a due balance of its error of figure and color throughout the whole area of the objective.

But let us examine briefly into the validity of the conclusions of the jury in another way. The report says: "Combinations of extremely extended angles of aperture are excellent in developing one class of test objects, viz., minute lines or dots on plane surfaces," etc., etc. From the texture of this sentence, we should be led to suppose that test objects were things of no general interest, apart from the uses to which they are applied, and that objectives might be made which would give a perfect resolution of the structure of these objects, yet would not possess advantages equal to those of smaller aperture, in the examination of objects of natural history, or minute anatomy. Such is a fair translation of the meaning of the sentence. Now, as all of these same test objects in general use are natural objects, whose texture and markings it is of interest to the naturalist to make out, and as the same objectives which will enable him to resolve them, will enable him to bring out the structures of all other objects to the same degree of minuteness, it is evident that the conclusions of the jury are not only erroneous in denying known laws, but are equally false in the assertion of principles that have no existence. Indeed, on the next page of the report the jury in making mention of the objectives of Smith and Beck, say: "The half-inch* focus of 70° aperture is a wonderfully fine combination, easily showing objects considered difficult for one-eighth inch focal length a little more than a year since, and bearing the application of the higher eye-pieces in an unprecedented manner." Now it might be fairly asked of the jury to explain upon what peculiarity this excellence of the half-inch objective (the one of largest angle of aperture for this power, mentioned in the report) depended. It is perfectly notorious, so far as mere *definitions* are concerned, that the best artists of the present day have not advanced one step beyond the triple achromatics of Tulley, made in 1823. It is equally true that the discoveries of Lester, while they have enabled artists to obtain larger angles of aperture, with due balance of the errors of color and figure, have added nothing to the *defining* power of objectives.

But a still more searching objection to the conclusions of the jury must be made; and one, too, drawn from the very departments of research to which they have made especial reference. As above stated, the focus of a perfect objective is, as nearly as may be, a mathematical plane, and, such being the case, all objects of whatever kind, shape, size, or character, are examined under nearly the same circumstances—the structure only of such objects as are accurately in focus, can be examined with any certainty. Nearly all of the objects and structures which are made the subjects of study or investigation by the microscopic anatomist and naturalist, are transparent ones, and are examined by transmitted light. In order to rid the examination of these objects of the confusion of varying refractions, diffractions, and interferences, it is necessary that nearly all of them should be mounted in fluid or balsam. The advantages thus obtained are sufficiently great to compensate for the defects belonging to this method of examination. One of its defects, however, is frequently felt as a serious inconvenience. The refractive power of the fluids used is often so nearly that of the structures themselves, that, with objectives of feeble penetrating power, they may disappear altogether, or, at most, exhibit a bare, meager outline. For this defect there is but one remedy—that of using objectives of large angle of aperture, which alone possess the capacity to take up pencils of high incidence, and under which the slight differences in refractive powers become most apparent. As by far the greater share of all microscopic objects are of the character above indicated, the superiority of objectives of the largest attainable angle of aperture is so evident, alike from the sound deductions of theory and the indisputable test of all past experience, that the only wonder is that any doubt should have been entertained on the subject. We will, therefore, close these lengthy strictures with the expression of the hope that, hereafter, the deficiencies of the microscope may be traced to their true cause—that of deficient skill in the artists who furnish them."

---

* Really two-fifths of an inch.

We have arrived now at the period when Mr. Spencer's microscopical objectives were considered as perhaps the best in the world, and the last notice I shall present is an extract from a letter of T. R. Robinson, D. D., of Dublin, Ireland, and President of the Royal Irish Academy, and who had devised the best method of measuring angular aperture. This letter was written to Prof. Bailey, and is dated March 6th, 1855. The angular aperture of the objective mentioned was, as measured the old way, about 170°:

"In reference to the performance of Mr. Spencer's objective (a $\frac{1}{12}$), Mr. Bergin (the owner) desires me to state that he had written a letter of some length on this matter, dated June 29th, but in consequence of his being taken ill the following morning, it was not posted until February 16th. I trust that you have long ere this received it. May I take the liberty of adding to what he has stated that it is the best objective *I* have ever seen. I have only met with one that compares with it in *sharpness* of definition, a *sixth* by Ross, 132° aperture. Of course the enormous aperture of Spencer prevents any other comparison, and the great working distance is equally remarkable. In this it contrasts strongly with some European objectives not exceeding 160°, which cannot be used without covers of unusual thinness. If I might wish for any improvement, it is that Mr. Spencer would put some graduation to the compensation, which in many cases is a great convenience, and which I have had applied to several of mine with advantage."

In the notice of Frey's book on the Microscope, in the *American Journal of Microscopy*, vol. V., May, 1880, p. 120, occurs the following sentence : "In regard to cover corrections, Frey perpetuates a very ^{e}common error. He says: 'With Hartnack's arrangement after each correction of the objective, the focus is naturally to be adjusted; this is not the case, however, with those of the English opticians, in which the position of the lowermost lens remains unaltered during the correction.' (Page 58.) The device here attributed to the English opticians was invented by Mr. Tolles." It is hardly necessary to remark, that a readjustment of focus is always necessary, no matter what device may be used for the cover correction.

To this statement Mr. Spencer prepared the following reply, but for some reason it was not sent. I can vouch for the truth of the statement. As first made, the whole tube and lenses were rotated, but the front lens did not change its position relative to the cover, only the interior systems moved. Mountings of this kind are yet in existence :

> "*Mr. Editor*:—I have hitherto refrained from asking attention to an error into which you and others have fallen, delaying to do so under the expectation that in justice and comity a correction would ere this be made by other hands than mine.
>
> In your notice of the recent edition of Frey's work on the Microscope, and in correction of the mistake therein of crediting to English opticians the device of making cover-adjustment by the movement of the *inner* systems of an objective, you have fallen into the mistake of claiming it for Mr. Tolles.
>
> This form of mounting was suggested to and devised by me, in consequence of breaking several valuable slides by the movement of the front system for adjustment; and was made whilst Mr. Tolles was employed by me under instruction, and before he had made or mounted any adjustable objectives.
>
> In the first arrangement for making this adjustment—by the movement of the inner instead of outer systems (the latter being the English mode),—the outer tube and front system were rotated axially (without longitudinal motion), and were without graduated collar. Subsequently, the outer tube remained fixed, and the movement of the inner systems was made by a graduated, revolving collar, as in the present mounting."

While Mr. Spencer was thus perfecting the microscopical objective, he was by no means neglecting the telescope. About the year 1854 he formed a partnership with Mr. A. K. Eaton, and in addition to the microscopical work, they completed various achromatic telescopes, of apertures varying from 3 to 5 inches, and in 1865 they finished the large Equatorial for Hamilton College, having an object glass of 13½ inches diameter, and a focal length of 16 feet. This was then the largest telescope in this country, and the contract price was $10,000, and in its performance it compared favorably with the best Munich instruments.

In 1856, Messrs. Spencer and Eaton entered into a contract with the Trustees of the Dudley Observatory at Albany to construct a magnificent heliometer, for the sum of $14,500. This was to embody some new principles, suggested by Mr. Spencer and approved by Dr. B. A. Gould, the director of the Observatory, and it was decided to send Mr. Spencer abroad to visit the principal workshops of Europe and the celebrated observatories, in order that the instrument might surpass anything hitherto made. Mr. Spencer sailed from New York in March, 1856, and was accompanied by his friend John E. Gavitt, the well-known engraver and microscopist, then living in Albany. While he was absent the optical department of the Canastota establishment was managed by Mr. Robert B. Tolles, now of Boston, and who had been for some years a pupil of Mr. Spencer.

Mr. Spencer spent some six months abroad, and studied very closely the construction of the large heliometer at Oxford, and made numerous friends among the scientific men of England and the Continent; but the heliometer was never built. An unfortunate and very bitter controversy arose between some of the Trustees of the Observatory and Dr. Gould, the merits of which need not here be discussed. The result was that the work of the Observatory was suspended for years, and it has never been, and it is doubtful if it ever can be, of that service to astronomical science which the founders had fondly hoped.

The partnership between Spencer and Eaton was dissolved after a few years, and Mr. Spencer, with the aid of his sons, still carried on the business, until the year 1873. In the fall of this year occurred the disastrous fire at Canastota, which destroyed nearly every shop in the village, and the three hotels, and very nearly ruined Mr. Spencer. The fire commenced about 11 o'clock at night, in a building opposite to Mr. Spencer's shop, and lasted all night. So rapid was the spread of the fire, that Mr. Spencer lost nearly all his tools and machinery, the accumulations of many years of toil and skill, and a large amount of finished and unfinished work. Only the building was insured.

Crippled, but not wholly disheartened, Mr. Spencer and his sons, with what had been saved from destruction, commenced anew in a little barn for a workshop, and in 1875 they moved to Geneva, and for two years were connected with the Geneva Optical Works. After this, for about three years, the business was conducted under the name of C. A. Spencer & Sons. During this period they received, at the Paris Exposition, the highest award, a magnificent large gold medal, for excellence of their microscopical objectives. This award, the highest given, was the more gratifying, inasmuch as Spencer & Sons had not sent any objectives themselves to the Exposition. President Barnard, of Columbia College, New York, who was one of the United States Commissioners to the Exposition, had a number of their recent objectives, and was so convinced of their excellence that he took the necessary steps for entering them.

About a year before Mr. Spencer's death, his son, Mr. Herbert Spencer, commenced business in Geneva under his own name, while Mr. C. A. Spencer remained in the old shop. Already the evidence of an over-tasked constitution and a too long-continued strain upon his mental powers had become painfully evident to his friends. He did but little work, and spent most of the time in reading, occasionally experimenting with some new combination, but always genial and pleasant to such of his old friends as from time to time visited him to talk over the past, and discuss the future of microscopy, and science generally. His last illness was short. After confinement to his room for some three weeks, he died, peacefully, Wednesday evening, September 28th, 1881, sincerely mourned by every friend of the microscope.

The Society is indebted to Dr. C. H. Stowell and Louisa Reed Stowell, editors of *The Microscope*, for the portrait of Mr. Spencer accompanying this memoir. (See Frontispiece)—COM. ON PUB